MASS EXTINCTION: A COMMENTARY

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(Twenty-ninth annual address, delivered 19 March 1986)

ABSTRACT. Four neocatastrophist claims about mass extinction are currently being debated; they are that: 1, the late Cretaceous mass extinction was caused by large body impact; 2, as many as five other major extinctions were caused by impact; 3, the timing of extinction events since the Permian is uniformly periodic; and 4, the ages of impact craters on Earth are also periodic and in phase with the extinctions. Although strongly interconnected the four claims are independent in the sense that none depends on the others. Evidence for a link between impact and extinction is strong but still needs more confirmation through bed-by-bed and laboratory studies.

An important area for future research is the question of whether extinction is a continuous process, with the rate increasing at times of mass extinctions, or whether it is episodic at all scales. If the latter is shown to be generally true, then species are at risk of extinction only rarely during their existence and catastrophism, in the sense of isolated events of extreme stress, is indicated. This line of reasoning can only be considered an hypothesis for testing.

In a larger context, palaeontologists may benefit from a research strategy that looks to known Solar System and Galactic phenomena for predictions about environmental effects on earth. The recent success in the recognition of Milankovitch Cycles in the late Pleistocene record is an example of the potential of this research area.

DURING the past five years we have seen an enormous increase in debate about the causes of mass extinction. This has spilled over into disciplines far removed from palaeontology, including astrophysics, and the popular press has been covering the story avidly—perhaps too avidly. Furthermore, the extinction at the end of the Cretaceous has made a strange contribution to contemporary politics. The modelling of atmospheric effects of the putative meteorite impact has led directly to the concept of Nuclear Winter, at least in its American version.

The literature of the past five years has contained four strong claims about extinction:

1. That the terminal Cretaceous extinctions were caused by a collision between Earth and a comet or asteroid.
2. That as many as five other extinction events were similarly caused by large body impact.
3. That the principal extinction events of the Mesozoic and Cenozoic are uniformly periodic, with a spacing of 26–30 million years;
4. That the ages of the major impact craters on Earth are also uniformly periodic and in phase with the extinctions.

These four claims are independent of each other in the sense that any one could be wrong without affecting the others. The Cretaceous extinction could be impact-produced but not the others, or extinctions could be periodic but not driven by impacts. On the other hand, some people have chosen to put the four together in an integrated theory of mass extinction calling for a Solar System or Galactic 'clock' which causes periodic showers of comets producing both extinction and cratering at regular intervals.

To many palaeontologists the developments I have just described represent a most disturbing reversion to the worst kind of catastrophism: wild explanations calling on mysterious forces that nobody has ever really seen; hypothesis piled upon hypothesis with vanishingly little evidence for each one. Many palaeontologists are annoyed to see people from other disciplines charging into

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palaeontology with expensive instruments but no experience with rocks and fossils. To other palaeontologists, however, the new developments are plausible and supported by field and laboratory data. To these optimists, palaeontology is making new contributions to our knowledge of Earth history and to astronomy, to say nothing of increased understanding of extinction as it relates to the evolution of life.

In this essay, I will review briefly the present state of the four neo-catastrophist claims and then look a bit deeper into the general phenomenon of extinction in the fossil record. Finally, I will suggest a somewhat different approach to research into the possible extraterrestrial influences on past life.

EXTINCTION BY METEORITE IMPACT

The most important element in the current debate is the proposal of a very large impact at the Cretaceous-Tertiary boundary. This was based initially on the high concentrations of iridium in K-T boundary sections in Italy, Denmark, and New Zealand (Alvarez *et al.* 1980). The 1980 Alvarez paper made a powerful case by any normal standards. The reported anomalies were substantial and because concentrations of iridium in some meteorites are known to be high and because iridium is nearly absent in the Earth's crust, the Alvarez paper convinced many people immediately. Also, the finding was completely plausible because it is well known that meteorite impacts (comets plus asteroids) have been relatively common throughout the Phanerozoic. It is estimated that there have been about twelve collisions with bodies of 10 km or greater diameter during Phanerozoic time and up to 3600 collisions with 1 km objects (Shoemaker 1984).

If the iridium story had been an isolated research result, concerned only with meteorite impact, I doubt that anyone would have bothered to check the data or interpretation further. However, because the Alvarez group went on to urge that the impact caused the late Maastrichtian extinctions, the original interpretation of impact was hotly debated and the scientific community insisted on more evidence.

An irony is that the whole idea is not new. I refer in particular to a paper published in *Nature* in 1973 by the Nobel chemist, Harold C. Urey. Using the dates of Cenozoic tektites and comparing them statistically with the ages of extinctions, Urey argued that large body impacts were responsible for even relatively minor extinction events. He ended the paper with a prediction that tektites and other evidence of impact would ultimately be found at the top of the Cretaceous. Urey's statistical case was fairly strong. The strange thing is that the paper was virtually ignored.

Since 1980, iridium anomalies have been confirmed at scores of K-T boundary sections but more importantly, other kinds of evidence have been found. Data from osmium isotopes (Luck and Turekian 1983), probable microtektites (Smit and Klaver 1981; Montanari *et al.* 1983), and shocked quartz (Bohor *et al.* 1984) have provided what might be called 'overkill' in establishing the fact of an impact. This is not to say that there is complete unanimity. There are still dissenters, most notably C. B. Officer and C. L. Drake (1983, 1985) who have argued for a volcanic origin of the iridium and shocked quartz. All in all, however, I think that the case for extraterrestrial impact is very strong.

The causal link between the Cretaceous impact and mass extinction is much less secure. All we really have is a match or near-match in timing between two different events and there is much legitimate argument about how good this time match is. The problem of deciding on cause and effect comes down to arguments of probability. We have two relatively rare events in Earth history: a mass extinction and a very large impact. What are the chances that these two could have occurred at about the same time by pure chance? Fortunately, the coincidence problem can be tackled in a reasonably rigorous fashion. We can pose the following question: if there were twelve impacts of 10 km bodies during the Phanerozoic, as has been estimated, what is the probability that one of these should coincide with the Cretaceous extinctions, within the uncertainties of the geologic dating of the extinctions?

It turns out that if the extinctions are dated only to the nearest six million years, the average length of a post-Palaeozoic stage, the coincidence should be expected to occur by chance alone 10 % of the time, and the causal argument is not compelling. On the other hand, if the extinctions can be limited to the final two million years of the Cretaceous, the probability of chance co-occurrence is only 3 %, that is within the range normally considered to be statistically significant. Attaching a confidence level to the cause-and-effect hypothesis is thus possible but depends on the accuracy of the estimates of Phanerozoic impact rates and on our assessment of the dating of the mass extinction. The exercise emphasizes, among other things, the importance of improving stratigraphic control and of deciding whether the extinctions were sudden or gradual.

The best way to decide whether impacts cause extinctions is to see whether other extinctions are associated in time with evidence of large body impact. Five other cases have been alleged. Iridium anomalies have been found at the Eocene–Oligocene boundary (Alvarez *et al.* 1982; Ganapathy 1982), the Middle–Upper Jurassic boundary at the top of the Callovian (Brochwicz-Lewiński *et al.* 1984), the Permo-Triassic boundary (Sun *et al.* 1984), the Frasnian–Famennian boundary in the Devonian (Playford *et al.* 1984), and at the base of the Cambrian (Hsü 1986). The last of these may not, of course, be an extinction event. The Eocene and Jurassic cases are not really major mass extinctions, but they certainly qualify as points of important faunal turnover and are used as series boundaries. Other extraterrestrial signatures have also been found in these Eocene and Jurassic cases. There is the possibility, of course, that the iridium and other impact evidence is ubiquitous in the geologic record and that it is found only at extinctions because this is where people have been looking. This problem is less serious than it once was, however, because so much more systematic searching has been done throughout the column.

All of the cases listed have real problems. In the Jurassic and Devonian cases the iridium is found only in stromatolites, making it possible that we are dealing with a purely biological enrichment. The Permo-Triassic case is based on Chinese analyses and work on portions of the same samples in other laboratories has not revealed the iridium.

In summary the link between impact and extinction is likely but not confirmed beyond reasonable doubt.

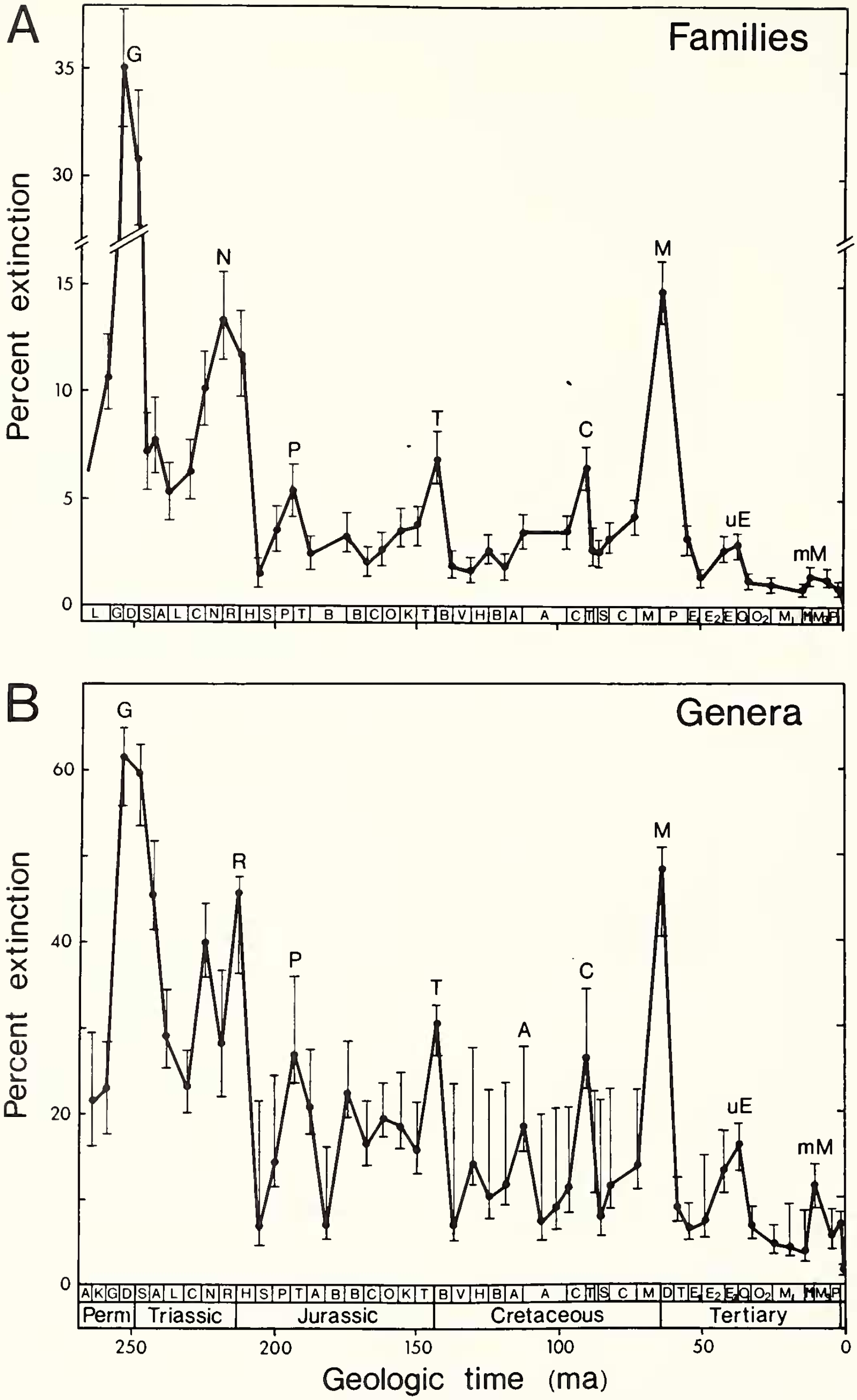
The proposition that extinction events are uniformly periodic (Raup and Sepkoski 1984, 1986) is alive and well although somewhat nervous. Periodicity is based on statistical inference from noisy and uncertain data so there are inevitable doubts and questions.

Text-fig. 1 shows the familial and generic extinction records from the late Permian onward. These are based on Sepkoski's compendium of stratigraphic ranges of marine families (Sepkoski 1982) and on his new compilation for genera (Raup and Sapkoski 1986). In the family data, eight events stand significantly above background: late Permian (Guadalupian or Dzulfian), late Triassic (Norian or Rhaetian), Pliensbachian, Tithonian, Cenomanian, Maastrichtian, Upper Eocene, and Middle Miocene. The error bars show the uncertainty in the extinction metric and are used to say whether the peaks are statistically above background. In the periodicity hypothesis, these eight events mark eight of the ten possible 26-ma cycles—with the missing events being in the Middle Jurassic and Lower Cretaceous. Interestingly, the Callovian iridium anomaly mentioned above is about where it should be for one of these gaps.

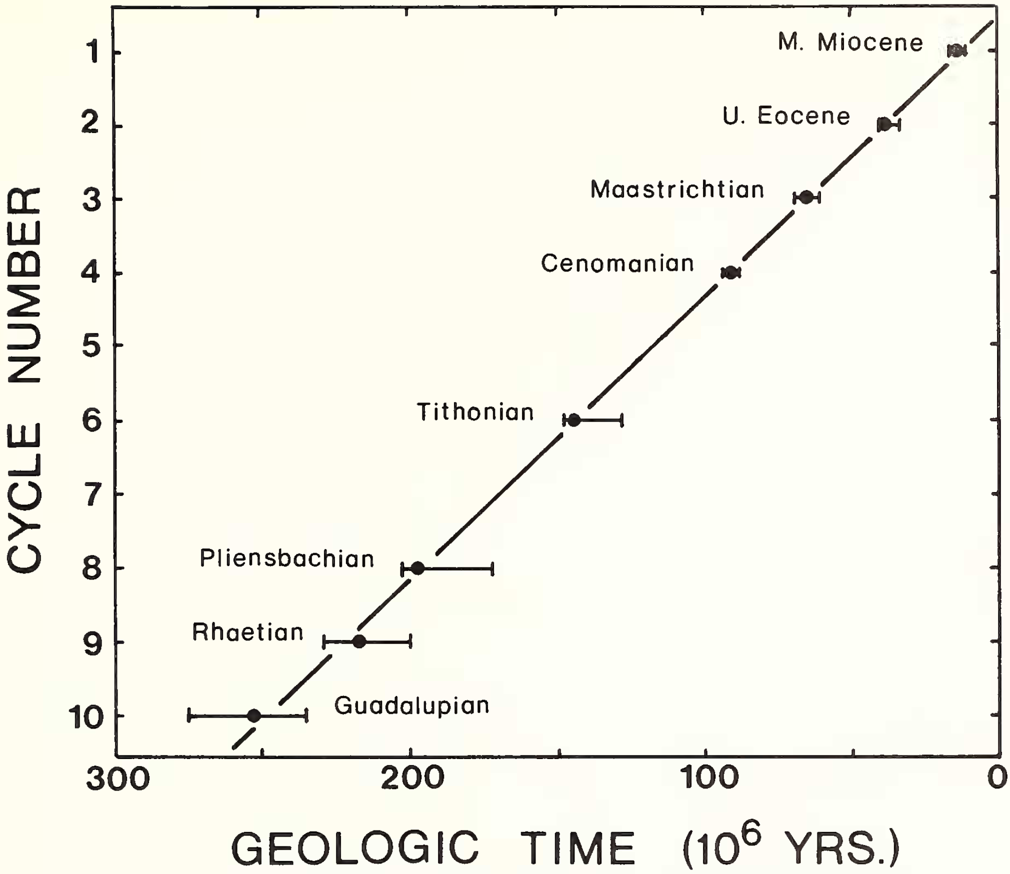
The generic data show the same peaks but the extinction events are generally more pronounced—even though the error bars are longer. The Upper Eocene and Middle Miocene events are much more convincing. In addition, the generic data show a small peak in the Aptian and this may fill the other gap in the 26-ma periodicity.

Text-fig. 2 summarizes the periodicity interpretation and shows the eight events in their proposed positions with worst case error bars for the stratigraphic and radiometic dating. The straight line is a 26-ma periodicity in best fit position. The youngest four events are the most accurately known. Only time will tell whether periodicity is confirmed by other kinds of data. To this end, Sepkoski is expanding the generic data set and is adding somewhat better stratigraphic resolution.

The analysis of the timing of these events is complex. Some people have claimed that the statistical analysis is flawed whereas others have accepted it. I think this is almost inevitable in the statistics



TEXT-FIG. 1. Per cent extinction for marine families (A) and genera (B) for the Permian to Recent interval. Extinction events which are identified by letters stand significantly above the local background. (From Raup and Sepkoski 1986.)



TEXT-FIG. 2. The ages of the eight principal extinction events in the Permian–Recent interval plotted against position in a perfect 26-ma periodicity (with two missing events). The straight line represents the 26-ma periodicity in best-fit position. The horizontal error bars are 'worst case' uncertainties in stratigraphic and radiometric dating. (Modified from Sepkoski and Raup 1986.)

of time series. One surprising reaction has come from some astronomers: they say that the periodicity is too perfect to accommodate some of the astronomical explanations that have been proposed.

Many palaeontologists and biostratigraphers have argued that the data on ranges of families and genera are inadequate to justify this sort of statistical analysis. People familiar with the taxonomy of a group appreciate all the guesswork and uncertainty and are appalled to see their taxa used as data for global, statistical analysis. Obviously, I think the taxonomic and stratigraphic data are worth using. Any data base of this sort can tolerate quite a bit of uncertainty as long as the uncertainty is not systematic. If it could be shown that monographic problems could produce a 26-ma cycle where none actually exists, we would have a real problem. But as long as the uncertainties are randomly distributed, their effect should only be to blur or cloud any signal or pattern that the data may have. Uncertain data can destroy a regular pattern but they cannot create one. This is analogous to any situation where one works with very large samples of uncertain data, and the problem actually pervades many fields of science. Consider the somewhat parallel situation in clinical medicine wherein medical histories of large numbers of patients are used to infer the causes of disease. For any given patient the information may contain large uncertainties and errors, yet the analysis of the whole data set often shows useful and important patterns. Closer to home, most normal biostratigraphic problems contain uncertain and conflicting data on fossil occurrences or time ranges, yet the whole system, operating in consensus fashion, is remarkably effective in defining Earth history. So, although the extinction data are sometimes very shaky, I suggest that they can and should be used to search for broad patterns and trends in the history of life.

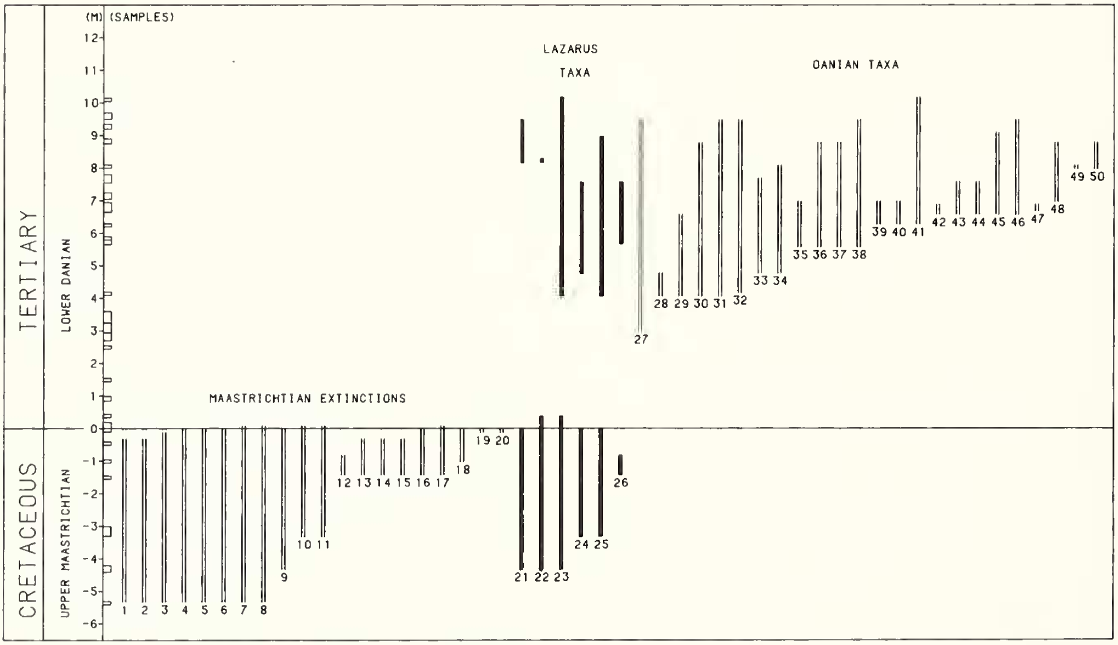
The periodicity question is closely related to the parallel question about the ages of impact craters (Rampino and Stothers 1984; Alvarez and Muller 1984) and here, too, the range of opinion is broad. Suffice it to say that the cratering data are not as robust as the extinction data.

In 1983 two papers were published claiming periodicities in reversals of the Earth's magnetic field compatible with the extinction and cratering records (Negi and Tiwari 1983; Mazaud *et al.* 1983). My own contribution (Raup 1985*a*) came to the same conclusion but was challenged on statistical grounds by Lutz (1985). Subsequently, however, two papers have appeared supporting the reversal periodicity (Pal and Creer 1986; Stothers 1986). This is important because there is some tempting evidence that large body impacts can cause magnetic reversals.

To summarize, the several claims about extinction are surrounded by uncertainty and controversy but I submit that they represent legitimate and sound science. Competent workers can disagree on some or all of them but there is no basis to discard the claims as unscientific. They are important hypotheses in the process of testing.

EXTINCTION: CONTINUOUS OR EPISODIC

There are many deficiencies in our knowledge of the extinction record but one of the most striking is our inability to say for sure whether extinction is best characterized as a continuous or episodic process. Are all species at risk of extinction all the time, with that risk fluctuating up and down through time, or does the risk of extinction alternate between high and low in a series of jumps? It has been traditional to use the continuous model and by this view, mass extinctions are simply



TEXT-FIG. 3. Stratigraphic ranges of fifty brachiopod species near the K–T boundary at Nye Klov in Denmark. (From Raup 1986; redrawn from Surlyk and Johansen 1984.)

intervals where the rates increase. On the other hand, the possibility of impact-caused extinction is more compatible with the episodic alternative.

Text-fig. 3 shows the record of fifty brachiopod species across the K–T boundary in one of the better Danish sections, based on work by Surlyk and Johansen (1984). The plot covers the brachiopods from about 5 m below the boundary to about 10 m above. The points at which collections were made are shown on the left. A group of species appears to go extinct precisely at the K–T boundary and several others drop out immediately below the boundary. The first question is whether the ones that go out below the boundary actually lived to the end of the Cretaceous but are missing in the boundary sample itself. This is important because it makes the difference between progressive extinction over a metre of section and sudden extinction at the last instant of the Cretaceous.

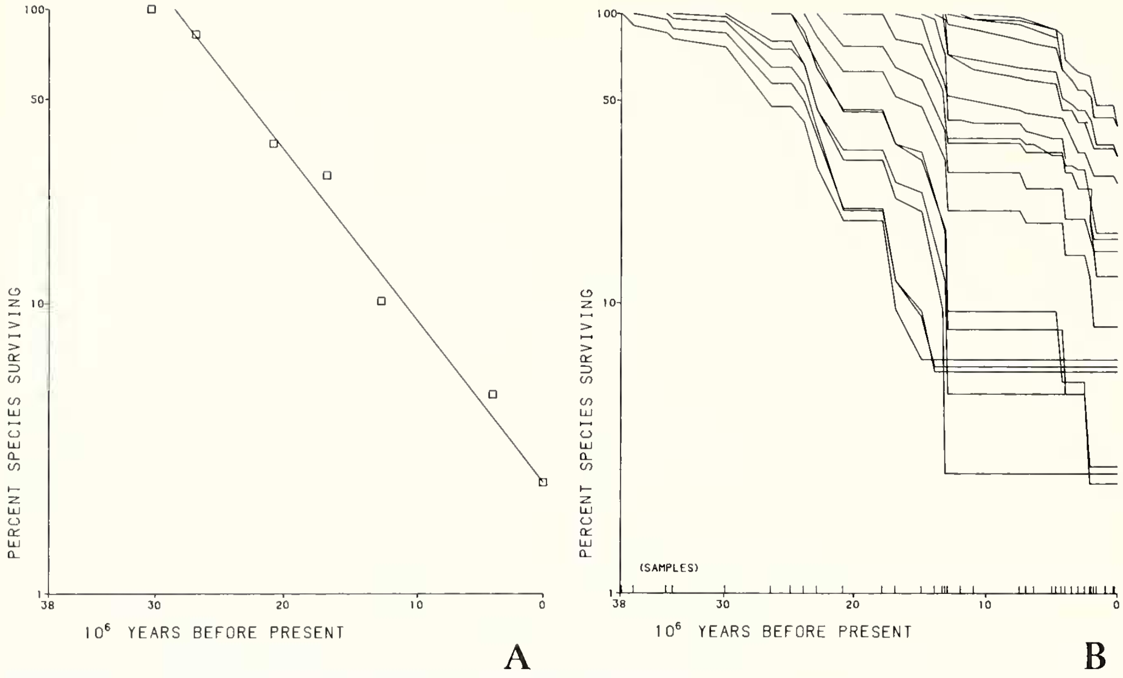
One could claim that the extinction is actually sharper than it looks because the rock record fails to record the actual last occurrence of these species. This backward smearing of an extinction event is known to all palaeontologists and has recently been called the Signor–Lipps Effect (Raup 1986). It is difficult to deal with because it depends so much on negative evidence. The Signor–Lipps Effect is a problem at all taxonomic and stratigraphic scales. At the family level, for example, the late Permian extinctions are spread over most of the final 10 million years of the Permian. The sedimentary record is poor and incomplete, however, and it is certainly possible that the true extinctions are clustered at a single point near the end of the Permian. It is difficult to say where the truth lies.

This plot also illustrates another common problem, called the Lazarus Effect by David Jablonski (in Flessa and Jablonski 1983). Notice that six species in the middle of the plot disappear at or near the K–T boundary, only to reappear several metres up into the Danian. Obviously, these species did not go extinct, assuming that their taxonomy is correct, but something happened to prevent their preservation in this section. These species are called Lazarus taxa. They raise the possibility that the apparently sharp episode of extinction at the K–T boundary could be an artifact of the same environmental or taphonomic change that eliminated the six Lazarus taxa. By this interpretation the extinctions may actually have taken place over a considerable time in the early Danian. The Lazarus Effect is also found at other scales. The absence of several important groups from the Early Triassic is an example. They are known to have survived the Permian extinctions but are not found again until later in the Triassic.

The problem comes down to whether we trust the fossil record as a complete record or not. As such, it is reminiscent of some of the arguments about gaps in the record that have surrounded the punctuated equilibrium question. There is some hope of controlling the problem. Jablonski (1986) has suggested that the record of Lazarus taxa can be used to evaluate the Signor–Lipps effect in a given case. That is, if there are several Lazarus taxa, as is the case here, we should be especially cautious about taking the record of last occurrences literally. The whole problem should be tackled with as much rigour as possible.

Let us consider the problem of continuous versus episodic extinction in a different way. Text-fig. 4A shows a cohort survivorship curve for species of planktonic foraminifera. The data come from a recent paper by Hoffman and Kitchell (1984). The horizontal scale is millions of years before present starting at the base of the Oligocene on the left. We start with the list of foraminiferid species present in the record at a point 30 ma ago and call this list a 'cohort'. This is plotted as 100 %. We then monitor the decay of this cohort by extinction of its constituent species through the remainder of the Cenozoic. The per cent of species remaining is plotted at approximately 5-ma intervals. By the end of the Cenozoic, the cohort is almost gone. Origination of new species during this time is ignored merely because we are interested only in extinction.

The fit of a straight line to the seven points is excellent and suggests a continuous process of extinction, with the species always being at risk. The straight line on the semi-logarithmic plot is analogous to radioactive decay, with a 'decay constant'—the slope of the line—indicating a species half-life of 5·3 ma. This is an excellent example of Van Valen's Law of constant extinction (Van Valen 1973; Raup 1975). This argues strongly for extinction as a continuous process, but when we add more data and plot them differently, an entirely different picture emerges.

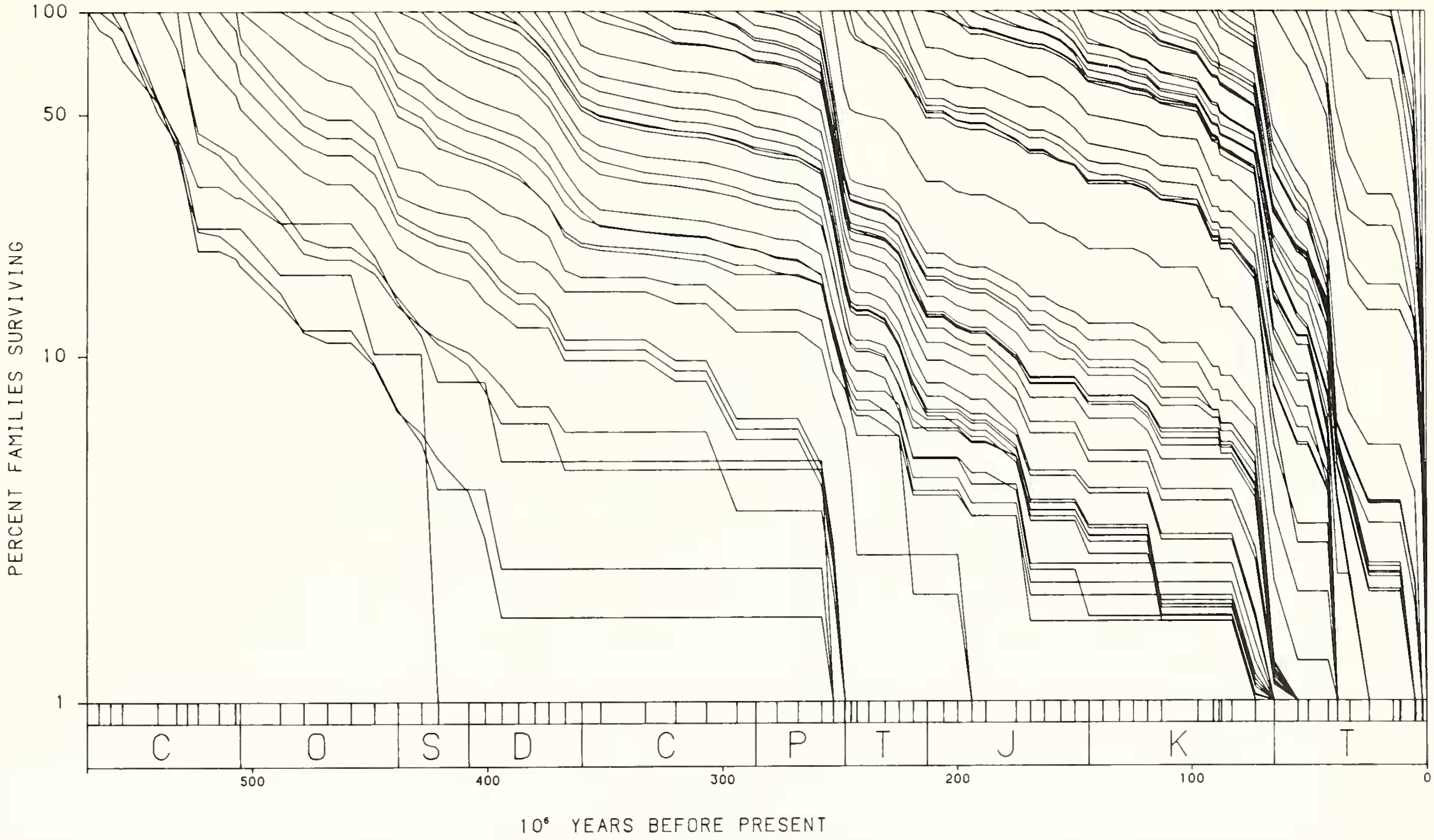


TEXT-FIG. 4. A, survivorship pattern for a polycohort of planktonic foraminiferal species from 30 ma BP (Early Oligocene) to the present, sampled at intervals of approximately 5 ma. The fitted straight line implies continuous, background extinction following Van Valen's Law. (From Raup 1986; data from Hoffman and Kitchell 1984.) B, survivorship patterns for a set of polycohorts of planktonic foraminiferal species from 30 ma BP to the present. Sampling points are shown on the inside of the lower, horizontal axis. The data points for survivorship have been connected by straight-line segments. This method of plotting emphasizes the episodic pattern of extinction in contrast to the method used in A. (From Raup 1986; redrawn from Hoffman and Kitchell 1984.)

Text-fig. 4B is also from Hoffman and Kitchell data but differs in three ways. First, more sampling points are used and these are indicated inside the horizontal axis. Secondly, more cohorts are followed, each differing in starting time. And thirdly, the points in the decay of each cohort are connected by straight line segments. Here, we see a sort of stair-step pattern. The horizontal parts (the treads) are times of no extinction and the steep portions (the risers) are times of simultaneous extinction of several species. There is an especially pronounced extinction event at about 12 ma BP where the risers are steep and high, the Middle Miocene event discussed above. The overall pattern suggests an episodic alternation between virtually no extinction and substantial extinction. By this interpretation the foraminifera experienced long periods of safety punctuated by short intervals of the proverbial panic.

The same exercise can be carried out at a larger scale. Text-fig. 5 shows a nest of cohort curves for about 2300 fossil marine families for the whole Phanerozoic (data from Sepkoski 1982). The biggest mass extinctions show as cliffs or scarps in this diagram. Especially striking are those at the ends of the Permian and Cretaceous but the graph shows many smaller events. It was this diagram that prompted Sepkoski and me to test the Mesozoic–Cenozoic record for periodicity of extinction. The Palaeozoic pattern is either less episodic or blurred by poor taxonomic and stratigraphic resolution.

The question of episodic versus continuous extinction is fundamental to questions of the causes of extinction. Are we dealing with progressive deterioration of environments, as is implied by most



TEXT-FIG. 5. Survivorship patterns for Phanerozoic polycohorts of marine families plotted in the manner of text-fig. 4B. Major extinction events appear as 'cliffs' and the intervals of negligible extinction appear as horizontal segments. (From Raup 1986; data from Sepkoski 1982.)

hypotheses of extinction based on sea-level or climatic change, or are we dealing with sudden, isolated events of environmental stress? The answer to this question does not necessarily involve extraterrestrial forces because the Earth is quite capable of producing sudden environmental shock by itself, but it is clear that extraterrestrial hypotheses are favoured by the episodic view of extinction.

I have no great confidence in any simple generalization on this question. In our present state of knowledge, it is too much in the eye of the beholder, but the problem represents a most important area for future research.

EVOLUTION IN A COSMIC ENVIRONMENT

Palaeontologists have usually adopted a defensive stance regarding proposals that cosmic factors may have influenced the history of life. When a proposal is made, the reaction is usually to look for flaws. The proposal is, in effect, 'guilty until proven innocent'. This is probably good practice in any field of science faced with radically new hypotheses, but it may mean that we miss some important opportunities for progress.

Let me propose, at least as a thought experiment, that we turn the situation around. Let us look at our cosmic environment to see what is going on that might be important to life on Earth and might leave a signature in the fossil record. With this approach, it may be possible to make specific predictions which can be tested in the fossil record.

The Solar System, and the Milky Way Galaxy of which it is a part, are dynamic and ever-changing. The Earth's orbit around the Sun changes over geologic time. The Sun's heat output probably changes substantially, with most estimates calling for as much as a 30 % increase since the early Pre-Cambrian. The Sun and other stars are constantly moving through space. We encounter interstellar clouds of gas and dust and we pass through the dense plane of the Galaxy every 30–33 ma. Other stars pass close to us on a random basis and these close encounters may well upset the orbits of Solar System comets. Nearby stars may explode producing supernovae close enough to Earth to produce environmental effects—and so on. There is a lot going on out there and some of this undoubtedly impinges on life on Earth. It would be surprising if it did not.

The time scales of these phenomena are often more appropriate to geologic data than to astronomical observation. The Phanerozoic has seen two complete rotations of the Galaxy, or two galactic years. At a smaller scale, the orbital changes in the Earth–Moon–Sun system produces cycles of between 20 000 and 400 000 years. And these shorter cycles have been spectacularly well recognized in recent work on Milankovitch Cycles in relation to ice ages over the past 700 000 years (Hays *et al.* 1976; Imbrie and Imbrie 1980).

Astronomers are surprisingly uncertain about most of the processes that operate on geologic time scales. An astrophysicist told me recently that if extinctions can be related to our position in the Galaxy, palaeontology will have provided the best available information on critical aspects of the form and dynamics of the Galaxy. In other words astronomy, that most sophisticated of all the so-called hard sciences, will welcome our help.

To be a bit more specific, we probably know most about large body impacts—whether or not their link to extinction can be confirmed. We have data from about 100 impact craters on Earth (Grieve 1982) as well as from those on the Moon, planets, and their satellites. Also, many present-day asteroids with Earth-crossing orbits have been sighted and their orbits plotted. Our knowledge of the comet population is not as good but it is growing.

From a palaeontological viewpoint, there are two main questions. What are the probable effects of an impact of a given size, and how many impacts have there been in the Phanerozoic? On the first question, we must rely on numerical simulations and on extrapolations from laboratory experiments. A great deal of this work has been done since the 1980 Alvarez paper and there is a reasonable consensus that impact by an object 10 km in diameter would have devastating, global effects—either through choking the atmosphere with dust or by nitrogen oxide pollution or by other means. A 10 km object falling in the ocean, which has an average depth of only 5 km, would be like throwing a brick into a mud puddle. A 1 km object would also have large effects but there is no consensus on their magnitude. Most of the attention has been on the 10 km size because this is the size estimated from the iridium concentrations at the K–T boundary. The question of size is important because impacts by 1 km objects have been several times more common in the Phanerozoic than those by 10 km objects (Shoemaker 1984). Are we talking just about the five or six really major mass extinctions or about extinction events at the level of the stratigraphic series, stage, or zone? This is not yet clear.

In this context, a recent analysis of two Jurassic extinctions (Pliensbachian and Tithonian) by Hallam (1986) is of interest. Hallam presents strong evidence that these events were regional rather than global and argues that this indicates that causes other than comet or asteroid impact were operating. An alternative explanation is that these events resulted from the purely regional effects of relatively small impacting objects. Lacking positive evidence for impacts at these times, we cannot, of course, claim that they were caused by small impacts. My only point is that impacts need not produce global environmental effects and that small impacting objects are known to be more frequent than large ones. Although the Pliensbachian and Tithonian events were apparently only regional extinctions, they were sufficient to produce statistically significant peaks in global compilations of taxonomic data (Raup and Sepkoski 1986).

I have emphasized large body impacts and extinction but this is only part of the story. Our cosmic environment has many other possibilities for influencing life. The success of palaeontology with Milankovitch Cycles is an example. These cycles of orbital change have been known since Ptolemy, but their application to the geologic past was pure speculation until the work on Pleistocene climate and palaeoecology by Imbrie, Shackleton, and their co-workers. No significant extinctions are involved here but the results are nevertheless palaeontologically important.

Another example not involving extinction is the use of growth banding in fossils to deduce day length and the history of the Earth–Moon system (Wells 1963; Scrutton 1965). Although controversial in some aspects, this is a clear case of palaeontology being able to test predictions of astronomy in order to say something interesting about the history of the Solar System.

To summarize, there is every reason to think that processes and events in space have had influences on the ecology and evolution of life and that a more positive approach should yield results.

CONCLUSION

I have covered a wide range of extinction related topics. I hope that I have demonstrated that the extinction mania of the past five years stems from valid research questions and that palaeontologists can play a constructive and positive role. Most needed are detailed field studies to describe and define the extinction sequences as precisely as possible. These studies should be co-ordinated with cognate research in geochemistry, geophysics, and astronomy.

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